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*ARGON.**

IT is some three or four years since I had the honour of lecturing here one Friday evening upon the densities of oxygen and hydrogen gases, and upon the conclusions that might be drawn from the results. It is not necessary, therefore, that I should trouble you to-night with any detail as to the method by which gases can be accurately weighed. I must take that as known, merely mentioning that it is substantially the same as is used by all investigators nowadays, and introduced more than fifty years ago by Regnault. It was not until after that lecture that I turned my attention to nitrogen; and in the first instance I employed a method of preparing the gas which originated with Mr. Vernon Harcourt, of Oxford. In this method the oxygen of ordinary atmospheric air is got rid of with the aid of ammonia. Air is bubbled through liquid ammonia, and then passed through a red-hot tube. In its passage the oxygen of the air combines with the hydrogen of the ammonia, all the oxygen being in that way burnt up and converted into water. The excess of ammonia is subsequently absorbed with acid, and the water by ordinary desiccating agents. That method is very convenient; and, when I had obtained a few concordant results by means of it, I thought that the work was complete, and that the weight of nitrogen was satisfactorily determined. But then I reflected that it is always advisable to employ more than one method, and that the method that I had used—Mr. Vernon Harcourt's method —was not that which had been used by any of those who had preceded me in weighing nitrogen. The usual method consists in absorbing the oxygen of air by means of

*A Lecture given by Lord Rayleigh before the Royal Institution of Great Britain, on Friday, April 5, 1895. Reprinted from the official report.

red-hot copper; and I thought that I ought at least to give that method a trial, fully expecting to obtain forthwith a value in harmony with that already afforded by the ammonia method. The result, however, proved otherwise. The gas obtained by the copper method, as I may call it, proved to be one-thousandth part heavier than that obtained by the ammonia method; and, on repetition, that difference was only brought out more clearly. This was about three years ago. Then, in order, if possible, to get further light upon a discrepancy which puzzled me very much, and which, at that time, I regarded only with disgust and impatience, I published a letter in *Nature* inviting criticisms from chemists who might be interested in such questions. I obtained various useful suggestions, but none going to the root of the matter. Several persons who wrote to me privately were inclined to think that the explanation was to be sought in a partial dissociation of the nitrogen derived from ammonia. For, before going further, I ought to explain that, in the nitrogen obtained by the ammonia method, some—about a seventh part—is derived from the ammonia, the larger part, however, being derived as usual from the atmosphere. If the chemically derived nitrogen were partly dissociated into its component atoms, then the lightness of the gas so prepared would be explained.

The next step in the enquiry was, if possible, to exaggerate the discrepancy. One's instinct at first is to try to get rid of a discrepancy, but I believe that experience shows such an endeavor to be a mistake. What one ought to do is to magnify a small discrepancy with a view to finding out the explanation; and, as it appeared in the present case that the root of the discrepancy lay in the fact that part of the nitrogen prepared by the ammonia method was nitrogen out of ammonia, although the greater part remained of common origin in both cases, the application of the principal suggested a trial of the weight of nitrogen obtained wholly from ammonia. This could easily be done by substituting pure oxygen for atmospheric air in the ammonia method, so that the whole, instead of only a part, of the nitrogen collected should be derived from the ammonia itself. The discrepancy was at once magnified some five times. The nitrogen so obtained from ammonia proved to be about one-half per cent. lighter than nitrogen obtained in the ordinary way from the atmosphere, and which I may call for brevity 'atmospheric' nitrogen.

That result stood out pretty sharply from the first; but it was necessary to confirm it by comparison with nitrogen chemically derived in other ways. The table before you gives a summary of such results, the numbers being the weights in grams actually contained under standard conditions in the globe employed.

ATMOSPHERIC NITROGEN.

By hot copper (1892)	2.3103
By hot iron (1893)	2.3100
By ferrous hydrate (1894)	2.3102
Mean	2.3102

CHEMICAL NITROGEN.

From nitric oxide	2.3001
From nitrous oxide	2.2990
From ammonium nitrite purified at a red heat	2.2987
From urea	2.2985
From ammonium nitrite purified in the cold	2.2987
Mean	2.2990

The difference is about 11 milligrams, or about one-half per cent.; and it was sufficient to prove conclusively that the two kinds of nitrogen—the chemically derived nitrogen and the atmospheric nitrogen—differed in weight, and therefore, of course, in quality, for some reason hitherto unknown.

I need not spend time in explaining the various precautions that were necessary in order to establish surely that conclusion. One had to be on one's guard against im-

purities, especially against the presence of hydrogen, which might seriously lighten any gas in which it was contained. I believe, however, that the precautions taken were sufficient to exclude all questions of that sort, and the result, which I published about this time last year, stood sharply out, that the nitrogen obtained from chemical sources was different from the nitrogen obtained from the air.

Well, that difference, admitting it to be established, was sufficient to show that some hitherto unknown gas is involved in the matter. It might be that the new gas was dissociated nitrogen, contained in that which was too light, the chemical nitrogen —and at first that was the explanation to which I leaned; but certain experiments went a long way to discourage such a supposition. In the first place, chemical evidence—and in this matter I am greatly dependent upon the kindness of chemical friends—tends to show that, even if ordinary nitrogen could be dissociated at all into its component atoms, such atoms would not be likely to enjoy any very long continued existence. Even ozone goes slowly back to the more normal state of oxygen; and it was thought that dissociated nitrogen would have even a greater tendency to revert to the normal condition. The experiment suggested by that remark was as follows—to keep chemical nitrogen—the too light nitrogen which might be supposed to contain dissociated molecules—for a good while, and to examine whether it changed in density. Of course it would be useless to shut up gas in a globe and weigh it, and then, after an interval, to weigh it again, for there would be no opportunity for any change of weight to occur, even although the gas within the globe had undergone some chemical alteration. It is necessary to re-establish the standard conditions of temperature and pressure which are always understood when we speak of filling a globe with gas, for I need hardly say that filling a globe with gas is but a figure of speech. Everything depends upon the temperature and pressure at which you work. However, that obvious point being borne in mind, it was proved by experiment that the gas did not change in weight by standing for eight months—a result tending to show that the abnormal lightness was not the consequence of dissociation.

Further experiments were tried upon the action of the silent electric discharge—both upon the atmospheric nitrogen and upon the chemically derived nitrogen—but neither of them seemed to be sensibly affected by such treatment; so that, altogether, the balance of evidence seemed to incline against the hypothesis of abnormal lightness in the chemically derived nitrogen being due to dissociation, and to suggest strongly, as almost the only possible alternative, that there must be in atmospheric nitrogen some constituent heavier than true nitrogen.

At that point the question arose, What was the evidence that all the so-called nitrogen of the atmosphere was of one quality? And I remember—I think it was about this time last year, or a little earlier —putting the question to my colleague, Professor Dewar. His answer was that he doubted whether anything material had been done upon the matter since the time of Cavendish, and that I had better refer to Cavendish's original paper. The advice I quickly followed, and I was rather surprised to find that Cavendish had himself put this question quite as sharply as I could put it. Translated from the old-fashioned phraseology connected with the theory of phlogiston, his question was whether the inert ingredient of the air is really all of one kind, whether all the nitrogen of the air is really the same as the nitrogen of nitre. Cavendish not only asked

himself this question, but he endeavoured to answer it by an appeal to experiment.

I should like to show you Cavendish's experiment in something like its original form. He inverted a U tube filled with mercury, the legs standing in two separate mercury cups. He then passed up, so as to stand above the mercury, a mixture of nitrogen, or of air, and oxygen; and he caused an electric current from a frictional electrical machine like the one I have before me to pass from the mercury in the one leg to the mercury in the other, giving sparks across the intervening column of air. I do not propose to use a frictional machine to-night, but I will substitute for it one giving electricity of the same quality of the construction introduced by Mr. Wimshurst, of which we have a fine specimen in the Institution. It stands just outside the door of the theatre, and will supply an electric current along insulated wires, leading to the mercury cups; and, if we are successful, we shall cause sparks to pass through the small length of air included above the columns of mercury. There they are; and after a little time you will notice that the mercury rises, indicating that the gas is sensibly absorbed under the influence of the sparks and of a piece of potash floating on the mercury. It was by that means that Cavendish established his great discovery of the nature of the inert ingredient in the atmosphere, which we now call nitrogen; and, as I have said, Cavendish himself proposed the question, as distinctly as we can do, Is this inert ingredient all of one kind? and he proceeded to test that question. He found, after days and weeks of protracted experiment, that, for the most part, the nitrogen of the atmosphere was absorbed in this manner, and converted into nitrous acid; but that there was a small residue remaining after prolonged treatment with sparks, and a final absorption of the residual oxygen. That residue amounted to about $\frac{1}{120}$ part of the nitrogen taken; and Cavendish draws the conclusion that, if there be more than one inert ingredient in the atmosphere, at any rate the second ingredient is not contained to a greater extent than $\frac{1}{120}$ part.

I must not wait too long over the experiment. Mr. Gordon tells me that a certain amount of contraction has already occurred; and if we project the U upon the screen, we shall be able to verify the fact. It is only a question of time for the greater part of the gas to be taken up, as we have proved by preliminary experiments.

In what I have to say from this point onwards, I must be understood as speaking as much on behalf of Professor Ramsay as for myself. At the first, the work which we did was to a certain extent independent. Afterwards we worked in concert, and all that we have published in our joint names must be regarded as being equally the work of both of us. But, of course, Professor Ramsay must not be held responsible for any chemical blunder into which I may stumble to-night.

By his work and by mine the heavier ingredient in atmospheric nitrogen which was the origin of the discrepancy in the densities has been isolated, and we have given it the name of 'argon.' For this purpose we may use the original method of Cavendish, with the advantages of modern appliances. We can procure more powerful electric sparks than any which Cavendish could command by the use of the ordinary Ruhmkorff coil stimulated by a battery of Grove cells; and it is possible so to obtain evidence of the existence of argon. The oxidation of nitrogen by that method goes on pretty quickly. If you put some ordinary air, or, better still, a mixture of air and oxygen, in a tube in which electric sparks are made to pass for a certain time, then, in looking through the tube, you observe the well-known reddish-orange fumes of the oxides

of nitrogen. I will not take up time in going through the experiment, but will merely exhibit a tube already prepared (image on screen).

One can work more efficiently by employing the alternate currents from dynamo machines which are now at our command. In this institution we have the advantage of a public supply; and if I pass alternate currents originating in Deptford through this Ruhmkorff coil, which acts as what is now called a 'high potential transformer,' and allow sparks from the secondary to pass in an inverted test tube between platinum points, we shall be able to show in a comparatively short time a pretty rapid absorption of the gases. The electric current is led into the working chamber through bent glass tubes containing mercury, and provided at their inner extremities with platinum points. In this arrangement we avoid the risk, which would otherwise be serious, of a fracture just when we least desired it. I now start the sparks by switching on the Ruhmkorff to the alternate current supply; and, if you will take note of the level of the liquid representing the quantity of mixed gases included, I think you will see after, perhaps, a quarter of an hour that the liquid has very appreciably risen, owing to the union of the nitrogen and the oxygen gases under the influence of the electrical discharge, and subsequent absorption of the resulting compound by the alkaline liquid with which the gas space is enclosed.

By means of this little apparatus, which is very convenient for operations upon a moderate scale, such as for analysis of 'nitrogen' for the amount of argon that it may contain, we are able to get an absorption of about 80 cubic centimetres per hour, or about 4 inches along this test tube, when all is going well. In order, however, to obtain the isolation of argon on any considerable scale by means of the oxygen method, we must employ an apparatus still more enlarged. The isolation of argon requires the removal of nitrogen, and, indeed, of very large quantities of nitrogen, for, as it appears, the proportion of argon contained in atmospheric nitrogen is only about 1 per cent., so that for every litre of argon that you wish to get you must eat up some hundred litres of nitrogen. That, however, can be done upon an adequate scale by calling to our aid the powerful electric discharge now obtainable by means of the alternate current supply and high potential transformers.

In what I have done upon this subject I have had the advantage of the advice of Mr. Crookes, who some years ago drew special attention to the electric discharge or flame, and showed that many of its properties depended upon the fact that it had the power of causing, upon a considerable scale, a combination of the nitrogen and the oxygen of the air in which it was made.

I had first thought of showing in the lecture room the actual apparatus which I have employed for the concentration of argon; but the difficulty is that, as the apparatus has to be used, the working parts are almost invisible, and I came to the conclusion that it would really be more instructive as well as more convenient to show the parts isolated, a very little effort of imagination being then all that is required in order to reconstruct in the mind the actual arrangements employed.

First, as to the electric arc or flame itself. We have here a transformer made by Pike and Harris. It is not the one that I have used in practice; but it is convenient for certain purposes, and it can be connected by means of a switch with the alternate currents of 100 volts furnished by the Supply Company. The platinum terminals that you see here are modelled exactly upon the plan of those which have been employed in practice. I may say a word or two on the question of mounting. The terminals

require to be very massive on account of the the heat evolved. In this case they consist of platinum wire doubled upon itself six times. The platinums are continued by iron wires going through glass tubes, and attached at the ends to the copper leads. For better security, the tubes themselves are stopped at the lower ends with corks and charged with water, the advantage being that, when the whole arrangement is fitted by means of an indiarubber stopper into a closed vessel, you have a witness that, as long as the water remains in position, no leak can have occurred through the insulating tubes conveying the electrodes.

Now, if we switch on the current and approximate the points sufficiently, we get the electric flame. There you have it. It is, at present, showing a certain amount of soda. That in time would burn off. After the arc has once been struck, the platinums can be separated; and then you have two tongues of fire ascending almost independently of one another, but meeting above. Under the influence of such a flame the oxygen and the nitrogen of the air combine at a reasonable rate, and in this way the nitrogen is got rid of. It is now only a question of boxing up the gas in a closed space, where the argon concentrated by the combustion of the nitrogen can be collected. But there are difficulties to be encountered here. One cannot well use anything but a glass vessel. There is hardly any metal available that will withstand the action of strong caustic alkali and of the nitrous fumes resulting from the flame. One is practically limited to glass. The glass vessel employed is a large flask with a single neck, about half full of caustic alkali. The electrodes are carried through the neck by means of an indiarubber bung provided also with tubes for leading in the gas. The electric flame is situated at a distance of only about half an inch above the caustic alkali. In that way an efficient circulation is established; the hot gases as they rise from the flame strike the top, and then as they come around again in the course of the circulation they pass sufficiently close to the caustic alkali to insure an adequate removal of the nitrous fumes.

There is another point to be mentioned. It is necessary to keep the vessel cool; otherwise the heat would soon rise to such a point that there would be excessive generation of steam, and then the operation would come to a standstill. In order to meet this difficulty the upper part of the vessel is provided with a water-jacket, in which a circulation can be established. No doubt the glass is severely treated, but it seems to stand it it in a fairly amiable manner.

By means of an arrangement of this kind, taking nearly three-horse power from the electric supply, it is possible to consume nitrogen at a reasonable rate. The transformers actually used are the 'Hedgehog' transformers of Mr. Swinburne, intended to transform from 100 to 2400 volts. By Mr. Swinburne's advice I have used two such, the fine wires being in series so as to accumulate the electrical potential and the thick wires in parallel. The rate at which the mixed gases are absorbed is about seven litres per hour; and the apparatus, when once fairly started, works very well as a rule, going for many hours without attention. At times the arc has a trick of going out, and it then requires to be restarted by approximating the platinums. We have already worked 14 hours on end, and by the aid of one or two automatic appliances it would, I think, be possible, to continue operations day and night.

The gases, air and oxygen in about equal proportions, are mixed in a large gasholder, and are fed in automatically as required. The argon gradually accumulates; and when it is desired to stop operations the supply of nitrogen is cut off, and only pure oxygen allowed admittance. In this way

the remaining nitrogen is consumed, so that, finally, the working vessel is charged with a mixture of argon and oxygen only, from which the oxygen is removed by ordinary well-known chemical methods. I may mention that at the close of the operation, when the nitrogen is all gone, the arc changes its appearance and becomes of a brilliant blue colour.

I have said enough about this method, and I must now pass on to the alternative method which has been very successful in Professor Ramsay's hands—that of absorbing nitrogen by means of red-hot magnesium. By the kindness of Professor Ramsay and Mr. Matthews, his assistant, we have here the full scale apparatus before us almost exactly as they use it. On the left there is a reservoir of nitrogen derived from air by the simple removal of oxygen. The gas is then dried. Here it is bubbled through sulphuric acid. It then passes through a long tube made of hard glass and charged with magnesium in the form of thin turnings. During the passage of the gas over the magnesium at a bright red heat, the nitrogen is absorbed in a great degree, and the gas which finally passes through is immensely richer in argon than that which first enters the hot tube. At the present time you see a tolerably rapid bubbling on the left, indicative of the flow of atmospheric nitrogen into the combustion furnace; whereas, on the right, the outflow is very much slower. Care must be taken to prevent the heat rising to such a point as to soften the glass. The concentrated argon is collected in a second gasholder, and afterwards submitted to further treatment. The apparatus employed by Professor Ramsay in the subsequent treatment is exhibited in the diagram, and is very effective for its purpose; but I am afraid that the details of it would not readily be followed from any explanation that I could give in the time at my disposal. The principle consists in the circulation of the mixture of nitrogen and argon over hot magnesium, the gas being made to pass round and round until the nitrogen is effectively removed from it. At the end that operation, as in the case of the oxygen method, proceeds somewhat slowly. When the greater part of the nitrogen is gone, the remainder seems to be unwilling to follow, and it requires somewhat protracted treatment in order to be sure that the nitogen has wholly disappeared. When I say 'wholly disappeared,' that, perhaps, would be too much to say in any case. What we can say is that the spectrum test is adequate to show the presence, or at any rate to show the addition, of about one-and-a-half per cent. of nitrogen to argon as pure as we can get it; so that it is fair to argue that any nitrogen at that stage remaining in the argon is only a small fraction of one-and-a-half per cent.

I should have liked at this point to be able to give advice as to which of the two methods—the oxygen method or the magnesium method—is the easier and the more to be recommended; but I confess that I am quite at a loss to do so. One difficulty in the comparison arises from the fact that they have been in different hands. As far as I can estimate, the quantities of nitrogen eaten up in a given time are not very different. In that respect, perhaps, the magnesium method has some advantage; but, on the other hand, it may be said that the magnesium process requires a much closer supervision, so that, perhaps, fourteen hours of the oxygen method may not unfairly compare with eight hours or so of the magnesium method. In practice a great deal would depend upon whether in any particular laboratory alternate currents are available from a public supply. If the alternate currents are at hand, I think it may probably be the case that the oxygen method is the easier; but otherwise, the magnesium

method would, probably, be preferred, especially by chemists who are familiar with operations conducted in red-hot tubes.

I have here another experiment illustrative of the reaction between magnesium and nitrogen. Two rods of that metal are suitably mounted in an atmosphere of nitrogen, so arranged that we can bring them into contact and cause an electric arc to form between them. Under the action of the heat of the electric arc the nitrogen will combine with the magnesium; and if we had time to carry out the experiment we could demonstrate a rapid absorption of nitrogen by this method. When the experiment was first tried, I had hoped that it might be possible, by the aid of electricity, to start the action so effectively that the magnesium would continue to burn independently under its own developed heat in the atmosphere of nitrogen. Possibly, on a larger scale, something of this sort might succeed, but I bring it forward here only as an illustration. We turn on the electric current and bring the magnesiums together. You see a brilliant green light, indicating the vaporisation of the magnesium. Under the influence of the heat the magnesium burns, and there is collected in the glass vessel a certain amount of brownish-looking powder which consists mainly of the nitride of magnesium. Of course, if there is any oxygen present it has the preference, and the ordinary white oxide of magnesium is formed.

The gas thus isolated is proved to be inert by the very fact of its isolation. It refuses to combine under circumstances in which nitrogen, itself always considered very inert, does combine—both in the case of the oxygen treatment and in the case of the magnesium treatment; and these facts are, perhaps, almost enough to justify the name which we have suggested for it. But, in addition to this, it has been proved to be inert under a considerable variety of other conditions such as might have been expected to tempt it into combination. I will not recapitulate all the experiments which have been tried, almost entirely by Professor Ramsay, to induce the gas to combine. Hitherto, in our hands, it has not done so; and I may mention that recently, since the publication of the abstract of our paper read before the Royal Society, argon has been submitted to the action of titanium at a red heat, titanium being a metal having a great affinity for nitrogen, and that argon has resisted the temptation to which nitrogen succumbs. We never have asserted, and we do not now assert, that argon can under no circumstances be got to combine. That would, indeed, be a rash assertion for any one to venture upon; and only within the last few weeks there has been a most interesting announcement by M. Berthelot, of Paris, that, under the action of the silent electric discharge, argon can be absorbed when treated in contact with the vapor of benzine. Such a statement, coming from so great an authority, commands our attention; and if we accept the conclusion, as I suppose we must do, it will follow that argon has, under those circumstances, combined.

Argon is rather freely soluble in water. That is a thing that troubled us at first in trying to isolate the gas; because, when one was dealing with very small quantities, it seemed to be always disappearing. In trying to accumulate it we made no progress. After a sufficient quantity had been prepared, special experiments were made on solubility of argon in water. It has been found that argon, prepared both by the magnesium method and by the oxygen method, has about the same solubility in water as oxygen—some two-and-a-half times the solubility of nitrogen. This suggests, what has been verified by experiment, that the dissolved gases of water should contain a larger proportion of argon than does at-

mospheric nitrogen. I have here an apparatus of a somewhat rough description, which I have employed in experiments of this kind. The boiler employed consists of an old oil-can. The water is applied to it and drawn from it by coaxial tubes of metal. The incoming cold water flows through the outer annulus between the two tubes. The outgoing hot water passes through the inner tube, which ends in the interior of the vessel at a higher level. By means of this arrangement the heat of the water which has done its work is passed on to the incoming water not yet in operation, and in that way a limited amount of heat is made to bring up to the boil a very much larger quantity of water than would otherwise be possible, the greater part of the dissolved gases being liberated at the same time. These are collected in the ordinary way. What you see in this flask is dissolved air collected out of water in the course of the last three or four hours. Such gas, when treated as if it were atmospheric nitrogen, that is to say after removal of the oxygen and minor impurities, is found to be decidedly heavier than atmospheric nitrogen to such an extent as to indicate that the proportion of argon contained is about double. It is obvious, therefore, that the dissolved gases of water form a convenient source of argon, by which some of the labor of separation from air is obviated. During the last few weeks I have been supplied from Manchester by Mr. Macdougall, who has interested himself in this matter, with a quantity of dissolved gases obtained from the condensing water of his steam engine.

As to the spectrum, we have been indebted from the first to Mr. Crookes, and he has been good enough to-night to bring some tubes which he will operate, and which will show you at all events the light of the electric discharge in argon. I cannot show you the spectrum of argon, for unfortunately the amount of light from a vacuum tube is not sufficient for the projection of its spectrum. Under some circumstances the light is red, and under other circumstances it is blue. Of course when these lights are examined with the spectroscope—and they have been examined by Mr. Crookes with great care—the differences in the color of the light translate themselves into different groups of spectrum lines. We have before us Mr. Crookes' map, showing the two spectra upon a very large scale. The upper is the spectrum of the blue light; the lower is the spectrum of the red light; and it will be seen that they differ very greatly. Some lines are common to both; but a great many lines are seen only in the red, and others are seen only in the blue. It is astonishing to notice what trifling changes in the conditions of the discharge bring about such extensive alterations in the spectrum.

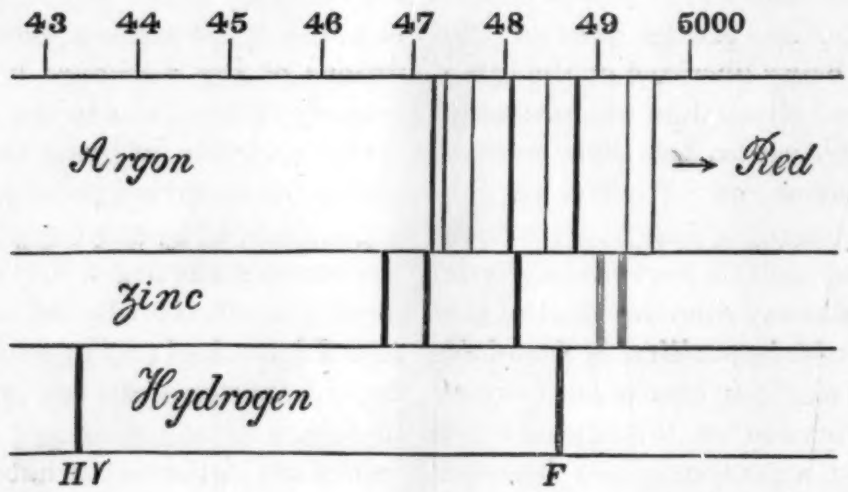
One question of great importance, upon which the spectrum throws light is: Is the argon derived by the oxygen method really the same as the argon derived by the magnesium method? By Mr. Crookes' kindness I have had an opportunity of examining the spectra of the two gases side by side, and such examination as I could make revealed no difference whatever in the two spectra, from which, I suppose, we may conclude either that the gases are absolutely the same, or, if they are not the same, that at any rate the ingredients by which they differ cannot be present in more than a small proportion in either of them.

My own observations upon the spectrum have been made principally at atmospheric pressure. In the ordinary process of sparking, the pressure is atmospheric, and if we wish to look at the spectrum we have nothing more to do than to include a jar in the circuit and to put a direct vision prism to the eye. At my request, Professor Schuster examined some tubes containing argon at atmospheric pressure prepared by the oxygen method, and I have here a diagram of

a characteristic group. He also placed upon the sketch some of the lines of zinc, which were very convenient as directing one exactly where to look. (See Fig.)

Within the last few days Mr. Crookes has charged a radiometer with argon. When held in the light from the electric lamp the vanes revolve rapidly. Argon is anomalous in many respects, but not, you see, in this.

Next, as to the density of argon. Professor Ramsay has made numerous and careful observations upon the density of the gas prepared by the magnesium method, and he finds a density of about 19.9 as compared with hydrogen. Equally satisfactory observations upon the gas derived by the oxygen method have not yet been made, but there is no reason to suppose that the density is different, such numbers as 19.7 having been obtained.



One of the most interesting matters in connection with argon, however, is what is known as the ratio of the specific heats. I must not stay to elaborate the questions involved, but it will be known to many who hear me that the velocity of sound in a gas depends upon the ratio of two specific heats—the specific heat of the gas measured at constant pressure, and the specific heat measured at constant volume. If we know the density of a gas, and also the velocity of sound in it, we are in a position to infer this ratio of specific heats; and by means of this method, Professor Ramsay has determined the ratio in the case of argon, arriving at the very remarkable result that the ratio of specific heats is represented by the number 1.65, approaching very closely to the theoretical limit, 1.67. The number 1.67 would indicate that the gas has no energy except energy of translation of its molecules. If there is any other energy than that, it would show itself by this number dropping below 1.67. Ordinary gases, oxygen, nitrogen, hydrogen, etc., do drop below, giving the number 1.4. Other gases drop lower still. If the ratio of specific heats is 1.65, practically 1.67, we may infer then that the whole energy of motion is translational; and from that it would seem to follow by arguments which, however, I must not stop to elaborate, that the gas must be of the kind called by chemists monatomic.

I had intended to say something of the operation of determining the ratio of specific heats, but time will not allow. The result is, no doubt, very awkward. Indeed, I have seen some indications that the anomalous properties of argon are brought as a kind of accusation against us. But we had

the very best intentions in the matter. The facts were too much for us; and all we can do now is to apologise for ourselves and for the gas.

Several questions may be asked, upon which I should like to say a word or two, if you will allow me to detain you a little longer. The first question (I do not know whether I need ask it) is, have we got hold of a new gas at all? I had thought that that might be passed over, but only this morning I read in a technical journal the suggestion that argon was our old friend nitrous oxide. Nitrous oxide has roughly the density of argon; but that, as far as I can see, is the only point of resemblance between them.

Well, supposing that there is a new gas, which I will not stop to discuss, because I think the spectrum alone would be enough to prove it, the next question that may be asked is, is it in the atmosphere? This matter naturally engaged our earnest attention at an early stage of the enquiry. I will only indicate in a few words the arguments which seem to us to show that the answer must be in the affirmative.

In the first place, if argon be not in the atmosphere, the original discrepancy of densities which formed the starting point of the investigation remains unexplained, and the discovery of the new gas has been made upon a false clue. Passing over that, we have the evidence from the blank experiments, in which nitrogen originally derived from chemical sources is treated either with oxygen or with magnesium, exactly as atmospheric nitrogen is treated. If we use atmospheric nitrogen we get a certain proportion of argon, about 1 per cent. If we treat chemical nitrogen in the same way we get, I will not say absolutely nothing, but a mere fraction of what we should get had atmospheric nitrogen been the subject. You may ask, why do we get any fraction at all from chemical nitrogen? It is not difficult to explain the small residue, because in the manipulation of the gases large quantities of water are used; and, as I have already explained, water dissolves argon somewhat freely. In the processes of manipulation some of the argon will come out of solution, and it remains after all the nitrogen has been consumed.

Another wholly distinct argument is founded upon the method of diffusion introduced by Graham. Graham showed that if you pass gas along porous tubes you alter the composition, if the gas is a mixture. The lighter constituents go more readily through the pores than do the heavier ones. The experiment takes this form. A number of tobacco pipes—eight in the actual arrangement—are joined together in series with india rubber junctions, and they are put in a space in which a vacuum can be made, so that the space outside the porous pipes is vacuous or approximately so. Through the pipes ordinary air is led. One end may be regarded as open to the atmosphere. The other end is connected with an aspirator so arranged that the gas collected is only some 2 per cent. of that which leaks through the porosities. The case is like that of an Australian river drying up almost to nothing in the course of its flow. Well, if we treat air in that way, collecting only the small residue which is less willing than the remainder to penetrate the porous walls, and then prepare 'nitrogen' from it by removal of oxygen and moisture, we obtain a gas heavier than atmospheric nitrogen, a result which proves that the ordinary nitrogen of the atmosphere is not a simple body, but is capable of being divided into parts by so simple an agent as the tobacco pipe.

If it be admitted that the gas is in the atmosphere, the further question arises as to its nature.

At this point I would wish to say a word of explanation. Neither in our original

announcement at Oxford, nor at any time since, until the 31st of January, did we utter a word suggesting that argon was an element; and it was only after the experiments upon the specific heats that we thought that we had sufficient to go upon in order to make any such suggestion in public. I will not insist that that observation is absolutely conclusive. It is certainly strong evidence. But the subject is difficult, and one that has given rise to some difference of opinion among physicists. At any rate, this property distinguishes argon very sharply from all the ordinary gases.

One question which occurred to us at the earliest stage of the enquiry, as soon as we knew that the density was not very different from 21, was the question of whether, possibly, argon could be a more condensed form of nitrogen, denoted chemically by the symbol N_3. There seem to be several difficulties in the way of this supposition. Would such a constitution be consistent with the ratio of specific heats (1.65)? That seems extremely doubtful. Another question is, Can the density be really as high as 21, the number required on the supposition of N_3? As to this matter, Professor Ramsay has repeated his measurements of density, and he finds that he cannot get even so high as 20. To suppose that the density of argon is really 21, and that it appears to be 20 in consequence of nitrogen still mixed with it, would be to suppose a contamination with nitrogen out of all proportion to what is probable. It would mean some 14 per cent. of nitrogen, whereas it seems that from one-and-a-half to two per cent. is easily enough detected by the spectroscope. Another question that may be asked is, Would N_3 require so much cooling to condense it as argon requires?

There is one other matter on which I would like to say a word—the question as to what N_3 would be like if we had it. There seems to be a great discrepancy of opinions. Some high authorities, among whom must be included, I see, the celebrated Mendeleef, consider that N_3 would be an exceptionally stable body; but most of the chemists with whom I have consulted are of opinion that N_3 would be explosive, or, at any rate, absolutely unstable. That is a question which may be left for the future to decide. We must not attempt to put these matters too positively. The balance of evidence still seems to be against the supposition that argon is N_3, but for my part I do not wish to dogmatise.

A few weeks ago we had an eloquent lecture from Professor Rücker on the life and work of the illustrious Helmholtz. It will be known to many that during the last few months of his life Helmholtz lay prostrate in a semi-paralyzed condition, forgetful of many things, but still retaining a keen interest in science. Some little while after his death we had a letter from his widow, in which she described how interested he had been in our preliminary announcement at Oxford upon this subject, and how he desired the account of it to be read to him over again. He added the remark: "I always thought that there must be something more in the atmosphere."

LLOYD MORGAN UPON INSTINCT.

In the last number of *Natural Science* Professor C. Lloyd Morgan gives a valuable synopsis of the various definitions of instinct which have been proposed by Darwin, Wallace, Romanes, James, Spencer and other writers upon this subject. He shows that surprisingly wide differences of opinion prevail and concludes that, "Since the question of origin is still *sub judice*, the definition should be purely descriptive, so as not to prejudge this question. And since the phenomena of instinct can only be rightly understood in their relation to automatism connate and acquired, to im-

pulse, to imitation and to intelligence, our definition of instinctive activities should find a place in a scheme of terminology." He sets forth such a scheme sending us in MSS. a number of additions and modifications which are embodied in the following table and abstract:

"It may be premised:

1. That the terms *congenital* and *acquired* are to be regarded as mutually exclusive. What is congenital is, as prior to individual experience, not acquired. What is acquired is, as the result of individual experience, not congenital.

2. That these terms apply to the individual, whether what is acquired by one individual may become congenital through inheritance in another individual, is a question of fact which is not to be settled by implications of terminology.

3. That the term *acquired* does not exclude an inherited potentiality of acquisition under the appropriate conditions, such inherited potentiality may be termed *innate*. What is acquired is a specialization of a vague and general innate potentiality.

4. That what is congenital and innate is inherent in the germ plasm of the fertilized ovum.

Congenital Movements and Activities: Those the performance of which is antecedent to individual experience; they may be performed either (a) at or very shortly after birth (*connate*) or (b) when the organism has undergone further development (*deferred*).

Congenital Automatism: The congenital physiological basis of those movements or activities which are antecedent to individual experience.

Physiological Rhythms: Congenital (or connate) rhythmic movements essential to the continuance of organic life.

Reflex Movements: Congenital, adaptive and coördinated responses of limbs or parts of the body; evoked by stimuli.

Random Movements: Congenital, more or less definite, but not specially adaptive movements of limbs or parts of the body; either centrally initiated or evoked by stimuli.

Instinctive Activities: Congenital, adaptive and coördinated activities of the organism as a whole; specific in character, but subject to variation analogous to that found in organic structures; similarly performed by all the members of the same more or less restricted group, in adaptation to special circumstances frequently recurring or essential to the continuance of the race; often periodic in development and serial in character.

Mimetic Movements and Activities: Due to individual imitation or similar movements or activities performed by others.

Impulse (Trieb): The affective or emotional condition, connate or acquired, under the influence of which a conscious organism is prompted to movement or activity, without reference to a conceived end or ideal.

Instinct: The congenital psychological impulse concerned in instinctive activities.

Control: The conscious inhibition or augmentation of movement or activity.

Intelligent Activities: Those due to individual control or guidance in the light of experience through association.

Motive: The affective or emotional condition under the influence of which a rational being is guided in the performance of deliberate acts.

Deliberate Acts: Those performed in distinct reference to a conceived end or ideal.

Habits: Organized groups of activities, stereotyped by repetition, and characteristic of a conscious organism at any particular stage of its existence.

Acquired Movements, Activities or Acts: Those the performance of which is the result of individual experience. Any modifications of congenital activities which result from experience are so far acquired.

Acquired Automatism: The individually modified physiological basis of the performance of acquired movements or activities which have been stereotyped by repetition."

Professor Morgan points out that there is some overlap in these definitions, but it is difficult to see how such overlaps are to be avoided. H. F. O.

SOME MEANDERING RIVERS OF WISCONSIN.

Two years ago Professor Davis* called attention to the wide meanders of the Osage river of Missouri. He said: "The meanders of the river are peculiar in not being like those of the Mississippi, spread upon a flat flood-plain. High spurs of the upland occupy the neck of land between every turn of the stream. Evidently the meanders are not of the ordinary kind." He explained the peculiar tortuous course of the river as an inheritance from an earlier cycle, during which the river had worn the land down to a surface of faint relief. The stream at that time swung to and fro in broad meanders developed on a wide flood-plain. The whole region was then somewhat elevated, and the stream again set to work to cut down its channel to the new baselevel. But the meandering course which it had acquired late in the preceding cycle was carried over into the new cycle of its life.

A recent visit to a part of the driftless area of Wisconsin, Lafayette and Grant counties, gave me an opportunity of observing a similar habit of some of the rivers of that region. The general surface of the country is that of a gently rolling plain, at an elevation of from 850 to 1000 feet, A. T. The interstream surfaces are broad and slightly undulating, but well drained. The surface rock, except in the immediate vicinity of the streams, is the Galena limestone. Occasionally the general level of the top of the country is broken by hills, which rise 200 to 300 feet above the general level. The highest of these are capped by the hard Niagara limestone; the lower by beds of the Cincinnati group. These hills form the so-called 'mounds,' of which, in the area visited, the Platte Mounds—1250–1300 feet, A. T.—are the highest. The hard Niagara limestone caps of these mounds are the remnants of beds which formerly stretched over all this region, and which has since been removed by denudation. To hills of this type Prof. Davis has given the name, Monadnocks.

The rocks of this region are nearly horizontal, and in general there is not a sharp contrast between the slant of the beds and the general slope of the upland surface. It seems, therefore, as if the upland might be a structural plain due to a resistant stratum, the Galena limestone, at the level of the upland—a stratum which had been revealed by denudation of the overlying beds. If this were the case, the upland level would be independent of any former baselevel. But such a conclusion does not seem to be admissible; although nearly horizontal, the limestone has been bent into gentle flexures, some of which are sufficient to bring the underlying Trenton limestone and St. Peter's sandstone up to the level of the upland surface. The plain is continuous across these low arches and bevels the edges of the gently inclined beds. Moreover, to the north of the outcrop of the Galena limestone, the upland plain bevels the gently inclined edges of the underlying formation, which there come to the surface. In that region, however, the plain is now more completely dissected than further south. Whatever correspondence exists between the inclination of the beds and the slope of the plain is fortuitous and not due to structure primarily. It is believed that this plain is a surface of denudation, the result of long continued erosion on a greater land mass when the land stood lower

*SCIENCE, April 28, 1893, vol. xxi., p. 225 et seq. SCIENCE, November 17, 1893, vol. xxii., p. 276 et seq.

than at present. The upland surface is believed to be an elevated peneplain.

It is now moderately dissected by valleys which along the larger rivers are from 100 to 200 feet deep. In comparison with the width of the gently undulating interstream surfaces these valleys are not very wide. The slopes are quite steep and locally form bluffs, but towards the top they pass by a graceful curve into the almost level upland. The present flood-plains along the bottoms of the valleys are generally from an eighth to a quarter of a mile in width. In terms of development the present valleys are well on towards maturity. The sharp narrow valleys of extreme youth are entirely absent. The rivers have made considerable progress in the present cycle in reducing the land mass to the level dependent on the grade of their channels, but the amount of work still to be done is vastly in excess of what has already been accomplished.

The three topographic features mentioned, namely, the broad undulating upland, with an elevation of from 850 to 1,000 feet; the few monadnocks rising above it, and the valleys cut into it, give a clue to the stages of geographic development of this region. The upland peneplain is a surface of denudation produced by long continued erosion, when the land mass stood lower than at present. This cycle of erosion lasted a long time and the baseleveling was almost completed. Very few monadnocks rose above the general plain. The cycle was ended by an uplift, which quickened the streams, restored to them their cutting powers, and compelled them to erode new valleys in the old peneplain. They have now cut down their channels until their ability to transport material is just about equal to the material which they have to carry. Rivers, the profiles of whose stream-channels are in this condition of equilibrium, have been called by Davis (SCIENCE, N. S., Vol. I., p. 176) graded rivers. The difference in the slope of the valley sides and the upland plain indicates a change of level before the excavation of the valleys and after the formation of the upland plain. The process by which the valleys are being formed is not a direct continuation of the process by which the gentle upland slopes were fashioned. The valleys were cut in the upland surface after it was elevated from the low position which it had during its formation.

Confirmatory evidence for this hypothesis is found in the winding courses of the valleys which now dissect this upland. Fever river was studied in the field, and the topographical atlas of the Wisconsin Geological Survey shows that the Platte, Little Platte, Grant and Pecatonica rivers have this same habit. If the geographical development of this region was as outlined above, the streams at the close of the earlier cycle must have possessed wide, flat valleys, with broad flood-plains, in which they meandered freely. The elevation of the land would have caused the streams to degrade their channels rapidly. In many cases the meanders on the flood-plain would have been superimposed upon the rock below, as the river bed was lowered. The valleys cut in the elevated peneplain would thus come to preserve, and, as pointed out by Winslow, also increase the meanders of the earlier cycle.

Such seems to have been the case with the Fever river. Its meanders have an average radius of a little less than half a mile, but they are by no means constant. Rock spurs of the upland project into each curve. The slopes on these spurs are generally gentler than on the outside of the curves, where the stream is often undercutting the base of the slope and increasing the meanders. Both open and close oxbows occur. The most marked of the close type of meanders was noted near Benton, where the river makes an almost closed sig-

moid curve, the halves of which are from one-half to three-fourths of a mile in diameter. The rock neck of land between the two ends of the closer curve is less than a hundred yards in width and rises about seventy feet above the stream.

Along Platte, Little Platte, Grant and Pecatonica rivers, larger streams than Fever river, the meanders are slightly large. on the average than along the smaller streams. Both open and close curves occur. Rock salients between 100 and 200 feet high project into the bow of each meander. Almost as complete a series of meander types can be found among the curves of the rock valleys of these rivers as along the broad flood-plains of other streams. Indeed, the small meanders of these rivers in their present flood-plains can readily be duplicated by the wider curves of the rock valley. There can be no reasonable doubt but that the meanders of these valleys are an inheritance from meanders developed on broad flood-plains in a previous cycle of erosion. So far as could be made out, these meanders are not due to difference in hardness or structure of the rocks of the region. The limestone does not present sufficiently marked differences of structure to account for these curves upon a theory of readjustment of courses due to the contrasts between hard and soft beds. Whatever differences exist are not distinctly such as to modify the courses of rivers, particularly in a manner such as to resemble so closely flood-plain meanders. Nor does it seem to be admissible to suppose that these curves are the perpetuation of meandering courses taken when the land first emerged from the sea bottom. Such a supposition presupposes too constant and stable a relationship, through an enormous lapse of time between all the forces which control erosion and determine the position of streams.

The sinuosities of these meanders may have been somewhat changed since the elevation of the peneplain. In places the increased velocity may have straightened the curves to some extent. In other instances the meanders have been somewhat increased. Such seems to have been the case near Benton, where the stream is now undercutting the narrow strip of land separating two parts of the curve. If this process continues, a cut-off will result.

In comparison with the Osage river, these streams are small and their meanders insignificant. But apart from size, the analogy between them is complete. They must be added to the growing list of streams known to be persisting in habits acquired under conditions which have long since disappeared.

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MISSOURI BOTANICAL GARDEN.

THE attention of botanists is called to the facilities afforded for research at the Missouri Botanical Garden. In establishing and endowing the Garden, its founder, Henry Shaw, desired not only to afford the general public pleasure, and information concerning decorative plants and their best use, and to provide for beginners the means of obtaining good training in botany and horticulture, but also to provide facilities for advanced research in botany and cognate sciences. For this purpose, additions are being made constantly to the number of species cultivated in the grounds and plant houses, and to the library and herbarium, and, as rapidly as it can be utilized, it is proposed to secure apparatus for work in vegetable physiology, etc., the policy being to secure a good general equipment in all lines of pure and applied botany, and to make this equipment as complete as possible for any special subject on which original work is undertaken by competent students.

A very large number of species, both

native and exotic, and of horticulturists' varieties, are cultivated in the Garden and Arboretum and the adjoining park, and the native flora easily accessible from St. Louis is large and varied. The herbarium, which includes nearly 250,000 specimens, is fairly representative of the vegetable life of Europe and the United States, and also contains a great many specimens from less accessible regions. It is especially rich in material illustrative of Cuscuta, Quercus, Coniferae, Vitis, Juncus, Agave, Yucca, Sagittaria, Epilobium, Rumex, Rhamnaceæ and other groups monographed by the late Dr. Engelmann or by attachés of the Garden. The herbarium is supplemented by a large collection of woods, including veneer transparencies and slides for the microscope. The library, containing about 8,000 volumes and 10,000 pamphlets, includes most of the standard periodicals and proceedings of learned bodies, a good collection of morphological and physiological works, nearly 500 carefully selected botanical volumes published before the period of Linnæus, an unusually large number of monographs of groups of cryptogams and flowering plants, and the entire manuscript notes and sketches representing the painstaking work of Engelmann.

The great variety of living plants represented in the Garden, and the large herbarium, including the collections of Bernhardi and Engelmann, render the Garden facilities exceptionally good for research in systematic botany, in which direction the library also is especially strong. The living collections and library likewise afford unusual opportunity for morphological, anatomical and physiological studies, while the plant house facilities for experimental work are steadily increasing. The E. Lewis Sturtevant Prelinnean library, in connection with the opportunity afforded for the cultivation of vegetables and other useful plants, is favorable also for the study of cultivated plants and the modifications they have undergone.

These facilities are freely placed at the disposal of professors of botany and other persons competent to carry on research work of value in botany or horticulture, subject only to such simple restrictions as are necessary to protect the property of the Garden from injury or loss. Persons who wish to make use of them are invited to correspond with the undersigned, outlining with as much detail as possible the work they desire to do at the Garden, and giving timely notice so that provision may be made for the study of special subjects. Those who have not published the results of original work are requested to state their preparation for the investigation they propose to undertake.

Under the rules of Washington University, persons entitled to candidacy in that institution for the Master's or Doctor's degree may elect botanical research work as a principal study for such degrees, if they can devote the requisite time to resident study.

WILLIAM TRELEASE, Director.

ST. LOUIS, MO.

SCIENTIFIC LITERATURE.

THE GEOLOGY OF THE SIERRA NEVADA.

Geologic Atlas of the United States. U. S. Geological Survey; J. W. POWELL, Director. *Sacramento Folio*, Geology by W. LINDGREN. *Placerville Folio*, Geology by W. LINDGREN and H. W. TURNER. *Jackson Folio*, Geology by H. W. TURNER. Washington, D. C. 1894.

These three sheets are the first installment of a series covering the gold belt of California which has been in course of preparation for several years by the officers of the Geological Survey. It is needless to say that they form a very important and welcome contribution to our knowledge of the geology of California. Since the collapse of the old State Survey under Whitney,

but little effort has been made by California to elucidate her economic geology, notwithstanding the liberal appropriations which the State Legislature makes regularly for the maintenance of a so-called 'Mining Bureau.' In the knowledge of her geologic resources, California is far behind many minor States of the Union. It is therefore fortunate that the Federal authorities have so steadily prosecuted the inquiry into the geology of the gold belt of the Sierra Nevada and of other portions of northern California. The sheets under review are the results of this work. They form part of the geologic atlas of the United States and they are among the first dozen of the entire series. The mechanical execution of the folios challenges the admiration of all familiar with such work. In the opinion of the writer they compare very advantageously with the best European efforts of a similar kind. It is gratifying to American pride to see the beginnings of so vast a scientific project as a geologic atlas of the United States realized in a manner so eminently satisfactory. If there exists a doubt in the minds of the geologists of the country, and in this case the geologists speak for the people, as to the ultimate success of the project, it is based on the fear that there may not be in the future, as there certainly has not been in the past, a proper coördination of the topographic and the geologic branches of the survey. A correct topographic base is the *sine qua non* of a good geologic map; and unfortunately the topographer's conception of a correct map, in the present state of his professional education, is not what it ought to be. Thorough and conscientiously executed topographic surveys are expected of the geological survey. The ambitious extension of the topographic surveys *far* in advance of geologic investigation, at a rate which not only absolutely precludes the possibility of thorough work but demoralizes the topographer, can only bring serious discomfiture to the Geological Survey as a government institution.

The Sacramento, Placerville and Jackson folios bring out clearly the salient features of a section which may be taken as typical for the western slope of the Sierra Nevada. The Sierra slope rises from the eastern edge of the Great Valley of California to the crest of the range, some 60 miles distant at an inclination of less than 2°. It presents the characters of a gently tilted plain which has been incisively dissected by the streams which traverse it. This slope is underlain by two very different assemblages of rocks. The first of these is composed of sedimentary and eruptive formations which have been intensely disturbed, metamorphosed and invaded by vast intrusions of granitic magma, forming a complex whose eroded surface serves as the basement upon which the second assemblage reposes in little disturbed attitudes. The older assemblage is designated in the folios the 'Bed-rock' series, and the newer, the 'Superjacent' series. Neither of these terms is felicitous, although the first is based on popular usage and will appeal to the mining community. The Bed-rock series comprises the rocks which are known popularly as the *auriferous slates*, together with their associated eruptives and irruptives, and also the granitic rocks which invaded the series as a whole at the close of the Jurassic. It would be better if these granitic intrusions were not classed in the same category with the auriferous slates as part of a 'series.' The auriferous slates comprise the *Calaveras* formation (Carboniferous, with possibly some older Paleozoic) and the *Mariposa* formation. In the earlier Sacramento and Placerville folios, which are chiefly Lindgren's work, the Mariposa formation is colored as Cretaceous, while in the later Jackson folio by Turner the same formation is colored as Jurassic. The reference of this formation to two different horizons can scarcely be

taken as indicative of decided difference of opinion between these two geologists, but rather of a rapid change of opinion on the part of the officers of the survey in consequence of the recent paleontological determinations of Hyatt, whose results were probably not available at the time the earlier folios went to press. The Mariposa formation is of economic importance as that in which occurs the zone of auriferous veining which constitutes the famous 'Mother Lode.'

In a field so overburdened with igneous rocks, contemporaneous and intrusive, geologists will readily understand that many problems arise which are not easily answered by the most earnest efforts of the field geologist. The lack of definite statements as to the structural relations of the various sedimentary and igneous formations indicates that these relations are obscure and difficult to determine. Still, a brief statement from Messrs. Turner and Lindgren as to the interpretation of their structural sections would have been a desirable addition to the letter press, which is limited strictly to historical, petrographic and economic geology. For example: Are the two belts of the Mariposa slates on the Jackson sheet essentially synclinal troughs with an anticline bringing up a belt of the lower Calaveras between them? If so, the structure is comparatively simple, and the great body of amphibolite schist, diabase and porphyrite probably represents volcanic accumulations chiefly intermediate in age between the Calaveras and the Mariposa, but perhaps passing up into the latter. Or is the region traversed by a great system of longitudinal faults? A discussion of these and other tectonic questions we may doubtless expect in more detailed reports upon the geology of the region. But something of the tectonic should find a place in the folios to help out the sections. While alluding to the igneous rocks it may be well to mention that the user of the geological map is handicapped by not having the effusive rocks discriminated from the intrusive on the color scale. From the text it is apparent that many of the igneous rocks are clearly intrusive, while others are effusive. This discrimination should be expressed graphically, as it is impossible to understand the structure without keeping it in mind. The doubtful rocks should be grouped apart from those which are clearly effusive or intrusive. An extra convention or two to express doubt or ignorance on particular points would greatly enhance the scientific value of our geological maps.

One of the most important features of the Sierra Nevada slope is the invasion of the Calaveras and Mariposa formations by the Sierra Nevada batholite. The relations of the older rocks to this invading magma are beautifully brought out by the careful mapping of Messrs. Turner and Lindgren. Petrographically, the rocks of this batholite are chiefly of a type intermediate between granite and diorite, and are therefore designated as *granodiorite*. Other important facies of the same magma are granite, gabbro and gabbro-diorite. These rocks appear as great intrusive areas in the midst of the auriferous slates and establish pronounced zones of contact metamorphism in the latter. Putting the three geologic sheets together, and bearing in mind the distribution of these same granitic rocks to the eastward and southeastward of the area mapped, it is difficult to resist the suggestion that these rocks underlie practically the whole of the Sierra slope beneath the rocks through which they project as isolated masses. In other words, the mapping suggests strongly that if the plane of truncation effected by erosion had been lower a much larger proportion of granite would have been exposed, and if higher less. If this suggestion be accepted it follows that the Calaveras and Mariposa formations must have reposed upon the granodiorite magma

as a crust, up into which the magma advanced, not only by displacement, but absorption. For we have no trace apparently of the original basement upon which the Calaveras formation was deposited. In these relations of batholite to disturbed and metamorphic crustal rocks we have a striking analogy with the relations which obtain between the Laurentian granites and the metamorphic rocks of the Ontarian system in the Lake Superior region. The amphibolites and other schists of 'auriferous slates' are petrographically the same as many of the schists of the Ontarian system.

The invasion of the Jurassic and earlier rocks by the Sierra Nevada batholite seems to have been accompanied, or perhaps preceded, by uplift and the development of mountain structure. During early Cretaceous time these mountains were profoundly eroded, for on the edge of the valley of California we find the Chico Cretaceous, the earliest of the 'Superjacent' series, reposing upon the worn surface of the granodiorite. The Chico is followed by the Ione and later Tertiary formations. In part contemporaneously with the Ione, but chiefly at a later period, there were spread over portions of the region important sheets of gravel. Associated with these are flows of rhyolite and andesite. The rhyolite flows serve as a means of separating the 'older' from the 'later' gravels. The andesitic flows were contemporaneous chiefly with the first of the later gravels. These gravels constitute the once famous placers of California. Since they were spread over the Sierra slope, the latter has been tilted so as to accentuate the grade and intensify the downward corrasion of the streams. As a consequence of this corrasion, we now find only remnants of the gravels and volcanic flows reposing on the tops of nearly flat ridges between the river gorges.

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On the [Harvest Mice] Species of the Genus Reithrodontomys. By J. A. ALLEN. 8° May 21, 1895. From Bull. American Museum of Natural History, New York (pp. 107–143).

Dr. Allen has just published a much needed revision of the Harvest Mice—a group of small mammals differing from other murine rodents in having the upper incisors deeply grooved. Since Dr. Allen's study is based on upwards of 900 specimens (two-thirds of which belong to the rich collection of the U. S. Department of Agriculture) it is probable that future researches will add little to the results here published, so far as the United States forms are concerned. The name of the common species of the Carolinas is changed from *humilis* to *lecontei.* Fifteen species and subspecies are recognized, 12 of which inhabit the southern and western parts of the United States. Seven of the United States forms are accorded full specific rank. One of these, *R. montanus* of Baird, is known from the type specimen only, which was collected in Colorado more than 40 years ago and is in very poor condition. When additional specimens are obtained from the type locality it will probably displace one of the other species. Another, *R. arizonensis* from the Chiricahua Mountains, is separated from *R. longicauda* of California, chiefly on geographic grounds. In the case of one of the subspecies admitted—*R. longicaudus pallidus*—it is not likely that Dr. Allen will be followed by other mammalogists. Respecting this form he says: "I find myself greatly embarrassed as to which of three courses to pursue in the matter, namely: (1) To refer *R. pallidus* to *R. longicauda* as a pure synonym of the latter; (2) to treat *R. pallidus* as one of several local phases of *R. longicauda;* (3) to let the name stand in a subspecific sense for a generally dispersed paler southern form of *R. longicauda*, as opposed to true *longicauda* of the region from about Monterey

and Merced counties northward. Through lack of material for properly working out the problem I have provisionally adopted the latter course."

Since he has 175 specimens that he regarded as typical *longicauda*, and 157 that he referred to subspecies *pallidus*, or 332 in all, and since these 332 specimens came from no less than 70 localities scattered over the single State of California, it is a little difficult to understand what he meant by 'lack of material for properly working out the problem.' Furthermore, an examination of the localities assigned to the two alleged forms shows them to be hopelessly mixed—both being recorded from the San Joaquin Valley, and both from the coast region north of Monterey!

One of the largest and most highly colored members of the group is a new form from Louisiana, collected by the field naturalists of the Department of Agriculture. It is a northern representative of *R. mexicanus* and is named, from its color, *R. mexicanus aurantius*.

The paper as a whole is a critical and painstaking study of an obscure group. It is based in the main on ample material and is particularly welcome as adding another genus to those recently revised by American mammalogists. C. H. M.

NOTES AND NEWS.

THE REMEDY FOR PEAR BLIGHT.

THE writer desires to announce that a satisfactory method of preventing pear blight has been discovered. After prolonged investigation the complete life history of the microbe (*Bacillus Amylovorous*) has been worked out. Most of the cases of blight either come to a definite termination in summer or else kill the tree. When this is the case the blight dies out completely, there being no source of supply for the germs the following spring. In certain cases where it is a sort of even battle between the host and parasite, or where late infections in the fall have not run their course before cold weather comes on, the blight keeps alive in the tree. When root pressure increases in the spring, such cases start into activity and serve as sources of infection for the new growth. The removal of these sources of infection is the preventive remedy for pear blight. The work is best performed in autumn after all late growth has ceased, but while the foliage is still on the trees. At this season the dead leaves which persist on the blighted branches serve admirably to attract attention to the points of danger. The work can be done at any time during the winter up to the time of the beginning of growth in spring. Cutting out the blight in summer is unsatisfactory on account of the continued appearance of new infections. The matter will be published in full in a bulletin from the Division of Vegetable Pathology.

M. B. WAITE,

DEPARTMENT OF AGRICULTURE.

THE NEW YORK BOTANIC GARDEN.

THE sum of $250,000 for the New York Botanic Garden has now been subscribed as follows:

J. P. Morgan	$25,000
Columbia College	25,000
Andrew Carnegie	25,000
C. Vanderbilt	25,000
J. D. Rockefeller	25,000
D. O. Mills	25,000
Judge A. Brown	25,000
Wm. E. Dodge	10,000
Jas. A. Scrymser	10,000
Wm. C. Schermerhorn	10,000
Ex-Judge C. P. Daly	5,000
O. Ottendorfer	5,000
Samuel Sloan	5,000
George J. Gould	5,000
Miss H. M. Gould	5,000
John S. Kennedy	5,000
Wm. Rockefeller	5,000
Jas. M. Constable	5,000

Morris K. Jesup	$2,500
Mrs. M. P. Dodge	1,000
Tiffany and Co.	1,000
Hugh N. Camp	500

The act of incorporation required that this amount be collected for an endowment. The city must now raise $500,000 by bonds for building purposes, and provide 250 acres of land in Bronx Park.

THE HELMHOLTZ MEMORIAL.

In addition to the subscriptions to the Helmholtz Memorial acknowledged in the issue of Science of May 31, the sum of $97 has been collected by Prof. Rood from officers of Columbia College and forwarded to the committee.

Ogden N. Rood,	$10
William Hallock,	5
H. Cushman,	5
R. Gordon,	3
H. C. Parker,	5
H. S. Curtis,	2
Asa S. Iglehart,	3
C. C. Trowbridge,	1
H. T. Wade,	1
J. H. Van Amringe,	10
F. R. Hutton,	5
F. B. Crocker,	5
J. K. Rees,	2
C. F. Chandler,	10
H. C. Bowen,	3
J. W. Burgess,	5
R. Mayo-Smith,	10
Wm. R. Ware,	5
Thomas Price,	2
H. T. Peck,	2
Livingston Farrand,	1
N. M. Butler,	1
James H. Hyslop,	1
	$97

GENERAL.

The thirty-fourth annual meeting of the National Educational Association of the United States will be held at Denver, Colorado, July 9th to 12th, 1895. The meeting promises to be the most important in the history of the Association. Among the large number of attractive addresses announced on the program are the address of the president, Professor Nicholas Murray Butler, on 'What Knowledge is of Most Worth,' and an address by Professor Joseph Le Conte on 'Effect of the Doctrine of Evolution upon Educational Theory and Practice.'

Mr. Archibald, president of the trustees of Syracuse University, has offered to be one of six men to build a hall of science costing about $150,000. The University has also been offered $10,000 and $100,000 towards a new medical college.

The University of Chicago announces that an *American Journal of Sociology* will be be issued bi-monthly from its press.

There are eleven candidates for the degree of Ph. D. at the University of Chicago —in Sociology and Geology each two, and in Philosophy, Greek, Latin, English History, Semetic and Chemistry each one.

Mrs. L. P. Babbott, of Brooklyn, has endowed a fellowship for post-graduate study at Vassar College.

During the coming year lectures on experimental psychology will be given by Dr. Scripture to the entire Junior Class, 300 members, of Yale College. Fifty undergraduates have elected special courses in the laboratory.

Colorado College will hold the fourth annual session of its summer school of science, philosophy and languages from July 15th to August 16th. Among the lecturers from other universities are Prof. Bessey, of Kansas; Prof. Lounsbury, of Yale, and Prof. James, of Harvard.

Part of the collection of birds given to the Museum of Comparative Zoölogy of Harvard University by Mr. W. E. D. Scott was ex-

hibited on June 18th. About 350 of the 3,200 birds have been mounted in 56 cases. Each case contains two or more birds of the same species, mounted in such a way that the character and ordinary habits and surroundings of the species are suggested without making the accessories of more apparent importance than the birds themselves.

The death is announced of Dr. Eliseyeff, known for his explorations in Asia and Africa.

A prize of $100 has been offered by a friend of Johns Hopkins University for the best essay by a student of the University upon the application of chemistry to the useful arts.

The Ethical Seminary for graduates in Harvard University will be conducted by Professor G. T. Ladd, of Yale University, in the absence of Professor Palmer during the coming year.

Additional courses of lectures will be given at Johns Hopkins University during the next academic year by Mr. G. K. Gilbert and Mr. Bailey Willis on geology, and by Dr. Frederick M. Warren, of Adelbert College on botany. The following appointments have also been made: Abraham Cohen, instructor in mathematics; Dr. Jacob H. Hollander, instructor in economics; Dr. Harry C. Jones, instructor in physical chemistry; Charles P. Singerfoos, an assistant in zoölogy and embryology.

Dr. John P. Lotzy has presented his herbarium of five thousand sheets to the Women's College of Baltimore.

The death is announced of Heinrich Geisburg, an authority on Westphalian history and archæology, in his seventy-seventh year.

Dr. Theophilus A. Wylie, Professor Emeritus in Indiana University, died recently at the age of eighty-five. He accepted the chair of natural philosophy and chemistry in Indiana University in 1837, in 1852 became professor of mathematics in Miami University, but returned to his former position after three years. He was transferred to the chair of languages in 1864, and withdrew from active work in 1886.

The presidency of the Columbian University of Washington has been offered to the Rev. B. L. Whitman, President of Colby University in Maine.

Professor Alexander Graham Bell has presented the Volta Bureau Library of Georgetown with the Scientific Library of the late Joseph Henry of the Smithsonian Institution, numbering 1,500 volumes.

At Harvard University Mr. G. A. Dorsey has been appointed instructor in anthropology, Mr. V. A. Wright instructor in descriptive geometry and stereotomy, and Dr. Alfred Schafer demonstrator of histology and embryology.

Prof. Valentine Ball, Director of the Museum of Science and Art of Dublin, died on June 17th, at the age of 52 years. He was elected a fellow of the Geological Society of London in 1874, fellow of the Royal Society in 1882, president of the Royal Geological Society of Ireland in 1882, and was professor of geology and mineralogy in the University of Dublin from 1881 to 1883. He was the author of works on the geology of India, and accounts of explorations in Afghanistan, Beloochistan, the Himalayas, etc.

Johns Hopkins University has received two gifts in memory of Prof. George H. Williams. His friends have given an oil portrait of Mr. Williams, and Mrs. Williams a sum of money sufficient to establish a lectureship in geology. Sir Archibald Geikie, Director of the Geological Survey of Ireland, has been invited to be first lecturer.

J. J. Hogan, mechanic and electrician in the Yale Psychological Laboratory, has invented a practicable device whereby the

high voltage city current is rendered readily available for low voltage instruments such as telegraph instruments, telephones, electric forks, bells, induction coils, etc. The General Electric Company has acquired patent rights. The details of the instrument will be made public as soon as the foreign patents are issued.

Dr. H. W. Williams, a distinguished opthalmological surgeon of Boston and author of several works on diseases of the eye, died at Boston on June 13th at the age of seventy-three years.

Prof. Michael Foster has now prepared an abridgement of his classical text-book of physiology, which in the sixth edition of five volumes had reached a size too large for the needs of the medical student. The abridged edition is published by Macmillan & Co. in an octave volume of about 1000 pages.

Mr. Erwin F. Smith of the Agricultural Department, has become one of the associate editors of *The American Naturalist*, taking charge of the department of vegetable physiology.

Macmillan & Co. announce the third edition of *Graduate Courses*, edited by C. A. Duniway, Harvard Graduate Club, assisted by graduate students representing twenty leading American universities. The work gives the advanced courses of instruction to be offered for 1895–6 in Barnard, Brown, Bryn Mawr, California, Chicago, Clark, Columbia, Cornell, Harvard, Johns Hopkins, Michigan, Minnesota, Pennsylvania, Princeton, Radcliffe, Stanford, Vanderbilt, Western Reserve, Wisconsin and Yale. Much valuable information is included regarding the conditions of advanced work at these universities.

At the commencement of the University of Pennsylvania a bronze bust of the late Professor Joseph Leidy was presented by Dr. Harrison Allen.

Sir Archibald Geikie has been elected a corresponding member of the Vienna Academy of Sciences.

Professor Simon Newcomb was elected on June 16th an associate academician of the Académie des Sciences to fill the vacancy caused by the death of von Helmholtz.

Mrs. Cornelia Phillips Spencer has received the degree of LL. D. from North Carolina State University.

At the summer meeting of the University Extension Society of Philadelphia, July 1–26, Courses in literature and history, psychology, music, biology, mathematics, civics and politics will be offered. The courses in science are as follows:

Psychology of the Normal Mind, by William Romaine Newbold, Ph. D., Penna.; Physiological Psychology of Adult and Child, by Lightner Witmer, Ph. D., Penna.; Hypnotic and Kindred Abnormal States of Mind, by Willian Romaine Newbold, Ph. D.; Anatomy and Physiology of the Nervous System, by Lightner Witmer, Ph, D.; Experimental Methods of Child Study, by Lightner Witmer, Ph. D.; Botany, by W. P. Wilson, Sc. D., Penna.; Systematic Botany, by J. M. Macfarlane, Sc. D., Penna.; Vertebrate Zoölogy, by Edward D. Cope, Ph. D., Penna.; Invertebrate Zoölogy, by J. S. Kingsley, S. D., Tufts; The Lower Plants, by Byron D. Halsted, Sc. D., Rutgers; Biology in Elementary Schools, by L. L. W. Wilson, Philadelphia Normal School; How Garden Varieties Originate, by L. H. Bailey, M. S., Cornell; Relation of Certain Plants to Political Economy, by George L. Goodale, LL. D., Harvard; The New Evolution, by Charles O. Whitman, Ph. D., Chicago; Higher Mathematics, Algebra, Modern Geometry, Etc., by Isaac J. Schwatt, Ph. D., Penna.

The first number of a series of *Princeton Contributions to Psychology* has been issued

from the University press, edited by J. Mark Baldwin and containing two articles reprinted from the Psychological Review: I. General Introduction—Psychology, past and present, by the editor; and II. Freedom and Psycho-genesis, by A. T. Ormond.

THE *Programme of the Department of Geology* of the University of Chicago for 1895–96 bears witness to the great strength of the department. Thirty-one courses are offered by the following officers of the department: Thomas C. Chamberlin, Head Professor of Geology; Rollin D. Salisbury, Professor of Geographic Geology; Joseph P. Iddings, Professor of Petrology; Richard A. F. Penrose, Jr., Professor of Economic Geology; William H. Holmes, Professor of Archæologic and Graphic Geology; Charles R. Van Hise, Non-resident Professor of Pre-Cambrian Geology; Oliver Cummings Farrington, Instructor in Determinative Mineralogy; Edmund C. Quereau, Tutor in Palæontologic Geology.

SOCIETIES AND ACADEMIES.

BIOLOGICAL SOCIETY OF WASHINGTON.

AT the meeting of June 1st Dr. C. Hart Merriam presented a paper on the Short-tailed Shrews of North America, stating that an examination of many specimens showed that the described species were only four, *Blarina brevicauda*, *B. carolinensis*, *B. parva and B. Berlandieri.* He discussed these and their distribution at some length, saying that each species was characteristic of one of the zoölogical divisions of North America.

Dr. G. Brown Goode made some remarks on the Location and Record of Natural Phenomena by a Method of Reference to Geographical Coördinates.

Dr. Gill presented a communication on The Relations of the Ancient and Modern *Ceratodontidæ*.

He commented on the unusual degree of interest connected with the Ceratodontids. The statement has been frequently made that *Ceratodus* is the oldest living generic type of fishes, and the identity of the living fishes so-called with the mesozoic species has been especially insisted on. The speaker, however, had denied such generic identity as early as 1878 on account of the difference in the form and plication of the dental plates, and had revived for the recent genus the name *Neoceratodus* given in mistake by Castelnau to a specimen of the genus. A new name, *Epiceratodus*, has recently been given by Teller to the same genus and must be abandoned. But Teller has given us useful data respecting the cranial characters of the mesozoic species, and we now have information sufficient at least to offer hints as to the relations of the ancient and modern forms. We can affirm positively that the recent Ceratodontids are very different from the mesozoic species; that consequently they should bear the name *Neoceratodus*, unless a still earlier one is applicable, and further that the differences between the living and long extinct species are enough to ever differentiate the two as distinct sub-families, the *Ceratodontinæ* including only extinct species and the *Neoceratodontinæ* being a recent type. The distinguishing characters of the two were given at length and derived from the dermal bones, the modification of the posterior region of the head, and the protrusion of the jaws. The ancient forms themselves belong to at least two genera: *Ceratodus*, typified by *C. Kaupii*, and *Anticeratodus*, typified by *C. Sturii*, of Teller. The latter is distinguished by the contiguity of the two palatine plates and their extended inner walls.

Professor Lester F. Ward exhibited specimens of the rhizomes of the Gama Grass, *Tripsacum dactyloides*, obtained at Great Falls, Md., on April 27th, which bore a striking resemblance to fossil forms described under the name of *Caulinites*, Brongn., and especially to *C. parisiensis*,

Brongn., from the Eocene of the Paris Basin. He exhibited figures of that species to show this resemblance.

The genus *Caulinites* was first figured by Desmarest, who supposed it to be a polyp and named it *Amphitoites parisiensis* in Nov. Bull. de Sci., Société Philomathique, tom. II., pl. 2. This figure was reproduced by Cuvier and Alex. Brongniart in Essai sur la Geographie Minéralogique des Environs de Paris, pl. II., figs. 10 A. and 10 B., 1811, and has been repeated in all later editions. A large number of very fine specimens were collected subsequently, and Adolphe Brongniart had no doubt but that it represented the impression of a plant. In his 'Tableau,' 1849, p. 86, he placed it under a plant genus which he renamed *Caulinites*, from the genus *Caulinia*, of de Candolle, a name antedated by *Posidonia*, Kön., an aquatic plant related to the river-weeds, *Potamogeton*, and sea wracks, *Zostera*, in the *Naiadaceæ*. When Watelet, in 1866, undertook the elaboration of all the material in the Paris Museum from the Eocene of the Paris Basin he devoted several plates to illustrating this and other species of the same genus.

Prof. Ward stated that when he saw the rhizomes he was forcibly struck with their resemblance to the figures of Desmarest and Watelet. A comparison of them showed that in many respects they were not only similar but practically identical, although among Watelet's figures are some which deviate considerably from this type. A large number of similar forms have been found in various deposits, chiefly Tertiary, throughout the world, and more than 50 species of *Caulinites* have been named, many of which will, of course, prove to be synonyms, while others depart so widely from the normal type that they will require to be excluded.

Prof. Ward said further that in 1887, Prof. Lesquereux described a species collected by Mr. Geo. F. Becker at Clear Lake, Cal., under the name of *C. Beckeri*. Proc. U. S. Nat. Mus. Vol. X., p. 36, pl. I, fig. 3, pl. II, figs. 1–4. Mr. Becker stated that he had supposed these rhizomes to belong to the common Tule, *Phragmites phragmites*, (L.) Karst., the deposit being a very recent one in the bed of a dried-up pond where the Tule was supposed to have grown as it now grows in those regions.

Prof. Ward remarked in conclusion that he had found other, similar, rhizomes washed up along the Potomac, but was unable to say to what plant they belonged, but enough is now known to make it certain that a considerable number of grasses, and perhaps rushes and other monocotyledonous plants, possess rhizomes with short joints resembling or practically identical with those of the genus *Caulinites*.

The Society then adjourned until October.

F. A. LUCAS, *Secretary*.

ENTOMOLOGICAL SOCIETY OF WASHINGTON.

THE 109th meeting was held June 6. Mr. Wm. H. Ashmead read a paper on the discovery of Elasmosoma Ruthe in America. This remarkable monotypical Microgasterine genus, the type species of which (*E. berolinense*) was collected in Europe many years ago in company with an ant, is supposed to be parasitic upon ants. Mr. Ashmead has found three species in America, one collected at Washington in 1889 by E. A. Schwarz; one at Fort Collins, Col., by C. F. Baker, and the third near Washington by Th. Pergande. The last species was found flying about the nest of *Camponotus melleus*, and the genus may be parasitic either upon ants or upon myrmecophilous beetles.

A paper by F. M. Webster entitled 'Notes on the Distribution of some Injurious Insects,' was read by the corresponding secretary. In this paper Prof. Webster criticised some of the details brought out by Mr. Howard in his paper on the geographical

distribution within the United States of certain insects injuring cultivated crops (Proc. Entom. Soc. Wash. III., No. 4), particularly in regard to the spread of injurious species into Ohio and their distribution in that State.

Mr. H. G. Hubbard exhibited specimens of the borings of Xyleborus and Platypus, Scolytid beetles, in orange wood. He described the habits of these beetles and showed that Platypus is capable of making extensive galleries of its own in hardwood trees. The nature of the food of these timber beetles was discussed. In addition to reviewing and confirming the observations of European writers, Mr. Hubbard described the so-called Ambrosia which nourishes the young, as welling up through the pores of the wood which are cut by the galleries, in the shape of minute white buttons, giving a tesselated appearance to the walls of the passages. The substance sometimes accumulates in the galleries, and when puddled by the larvæ resembles half-melted snow or slush. A growth of fungus forms upon the Ambrosia, and closing the mouth of the galleries causes them to fill up and suffocate the inmates. This method of treatment was found useful in Florida, to save from further injury the budded portion of trees killed back by the severe frost of February last. A piece of wire was pushed into the burrows as far as it would go and then cut off and left there.

As to the nature of Ambrosia, Mr. Hubbard made the conjecture that it is a ferment set up in the sap of the tree and augmented by the presence of the animals.

Mr. O. Heidemann exhibited specimens of *Coriscus flavomarginatus*, a brachypterus Nabid new to North America, which was collected at St. John's, New Brunswick, by the late Dr. Marx. Mr. Howard exhibited a female Scolia sent from Texas by Mr. E. A. Schwarz, and which had become, in some manner, impaled upon a sharp thorn, the thorn entering the middle of the face. It was a question whether the insect became so impaled by flying violently against the sharp point of the thorn, or whether it had been stuck there by a shrike. Mr. Frank Benton exhibited a comb of *Apis florea* which he had collected in Ceylon. This is the smallest species of Apis known. Curiously enough, the only two species of Apis which build in the open air, namely, *Apis florea* and *A. dorsata*, are the smallest and the largest species of the genus.

L. O. HOWARD,
Recording Secretary.

NEW YORK ACADEMY OF SCIENCE.

AT the meeting on May 27th Prof. Cattell described *Bodily and Mental Tests made on members of the Freshman Class of Columbia College* by him in conjunction with Dr. Farrand. About twenty-five observations and measurements were made on students entering college in 1894, and these will be repeated at the middle and end of the course. In describing the experiments especial attention was given to those of a more purely psychological nature, such as memory, accuracy of perception, sensitiveness to pain, reaction-time, rate of perception, imagery, etc., and some of the experiments were made on those present. Such experiments are of value to the individual student, as they give him information concerning his bodily and mental condition, and the effect of his college course upon these; they are also of use in increasing our exact knowledge of mental processes and their relation to bodily conditions.

Professor Rees exhibited a *Geodetic Theodolite* made by Wanschaff, of Berlin, for use in the Summer Class of Practical Geodesy at Columbia College. The telescope was 19½ inches in focal length with 2½ inch objective. The horizontal circle was 8 inches

in diameter and was read to single seconds of arc by two micrometer-microscopes. The graduations on the circle were microscopic and were seen easily in the reading microscopes. The telescope was provided with a small vertical circle $6\frac{1}{4}$ inches in diameter and reading by verniers to single minutes. The instrument was arranged for observations on Polaris for azimuth work.

J. F. Kemp, *Secretary.*

THE WISCONSIN ACADEMY OF SCIENCES, ARTS AND LETTERS.

The Wisconsin Academy of Sciences, Arts and Letters held its Summer meeting, on June 6th to 8th, 1895, at Milwaukee, Wis., under the auspices of the Natural History Society of Wisconsin, and the Presidency of Professor Charles R. Van Hise. In addition an address by President C. K. Adams and a number of other historical and sociological papers, the following were presented:

Address of Welcome: George W. Peckham, President of the Natural History Society of Wisconsin.

Opening address, 'Reforms in Germany after the Napoleonic Wars: C. K. Adams, President of the University of Wisconsin.

The relation of pooling to some phases of the transportation question: A. M. Simons.

The legal aspects of trusts: Edgar F. Strong. Read by title.

The forms spontaneously assumed by folk-songs: J. Comfort Fillmore.

Negro suffrage in Wisconsin: J. G. Gregory.

Some Observations on the Lateral Moraines at Devil's Lake: D. P. Nicholson.

Geology of Mts. Adam and Eve, Orange County, N. Y.; G. L. Collie.

Certain Uses of Topographical Maps: G. L. Collie.

The Production of Electrical Energy Directly From Carbon: A. J. Rogers.

A Contribution to the Mineralogy of Wisconsin: William H. Hobbs.

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Some New Occurrences of Minerals in Michigan and Montana; William H. Hobbs.

On a Diamond from Kohlsville, Wisconsin: William H. Hobbs.

From Pinene to Carvacrol: Edward Kremers.

A Dredge for Collecting Crustacea at Different Depths: C. Dwight Marsh.

Method of Determining the Coefficient of a Plankton Net: E. A. Birge.

The Pelagic Crustacea of Lake Mendota During the Winter and Spring of 1894–1895: E. A. Birge.

The Biological History of Daphnia Hyalina, Leydig: E. A. Birge.

The Periodic System as a Didatic Basis: Edward Kremers. Read by title.

Observed and Computed Precession: D. P. Blackstone. Read by title.

THE TEXAS ACADEMY OF SCIENCE.

The Law of Hypnotism: Prof. R. S. Hyer.

County Roads: Charles Corner, C. E.

On the Glycerine Method of Preserving Specimens for the Anatomical Museum: Dr. Wm. Keiller, F. R. C. S.

Texas Soils; a Preliminary Statement and Classification: E. T. Dumble.

Simultaneous Quadratic Equations: I. H. Bryant.

NEW BOOKS.

Geological Survey of Michigan. Lucius L. Hubbard, State Geologist. Vol. v. 181, 1893. pp. x+179. xxiv+100.

The Theory of Light. Thomas Preston. 2nd Edition. London and New York, Macmillan & Co. 1895. Pp. xvii+566. $5.00.

A Monograph of the Order of Oligochæta. Frank Evers Beddard. Oxford. Clarendon Press, New York, Macmillan & Co. 1895. Pp. xii+769. $12.50.

Report of the International Meteorological Congress Held at Chicago, Ill. Part II. Edited by Oliver L. Fassig. Washington, Weather Bureau. 1895. Pp. xvi+583.

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MAY 10, No. 19.

MAY 17, No. 20.